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Evaluating the German “Mini-Job” Reform Using a Natural Experiment*

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Abstract

Increasing work incentives for people with low income is a common topic in the policy debate across European countries. The “Mini-Job” reform in Germany had a similar motivation. We carry out an ex-post evaluation to identify the short-run effects of this reform. Our identification strategy uses an exogenous variation in the interview months in the SOEP, that allows us to distinguish groups that are affected by the reform from those who are not. To account for seasonal effects we additionally use a difference-in-differences strategy. Descriptives show that there is a post-reform increase in the number of Mini-jobs. However, we show that this increase can not be *causally* related to the reform, since the short-run effects are very limited only. Only single men seem to react immediately and increase secondary job holding.

Keywords: Evaluation, Natural Experiment, Difference-in-Differences, Marginal Employment.

JEL Classification: C25, H31, J68

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1 Introduction

As a response to persistently high unemployment rates, especially of low-skilled people, wage subsidies have been intensively discussed in European countries. Following the example of the Earned Income Tax Credit (EITC) introduced in the 70s in the US (see, e.g., Scholz, 1996), several European countries have introduced in-work benefits, tax credits, or subsidies to social security contributions (SSC) for working individuals. Examples are the Working Family Tax Credit (WFTC) in the UK (see, e.g., Blundell, Duncan, McCrae, and Meghir, 2000) and the French Prime Pour l'Emploi (see, e.g., Stancanelli, 2005).¹ The “Mini-Job” reform introduced in Germany in 2003 had a similar motivation. The main objective of this reform was to provide positive work incentives for people with low earnings potential by subsidising social security contributions. The government expected to achieve that goal by exempting labour income up to 400 euro from employees’ SSC and introducing a degressive subsidy for earnings between 401 and 800 euro. To be specific, this reform included three major changes compared to pre-reform regulations. First, the maximum amount for earnings exempted from SSC was increased from 325 to 400 euro. Jobs with earnings less than this threshold (marginal employment) are from then on labeled mini-jobs. Second, the previous *maximum* hours restriction (15 hours per week) was abolished. Third, income up to 400 euro per month from a mini-job held as a secondary job, which was fully taxable before the reform, is now exempted from SSC and income tax.²

The expected labour supply effects of wage subsidies, which consist of the effect on the participation as well as on the hours worked, depend on the design of the policy instrument and on various other institutional and economic factors (see, e.g., Blundell, 2000, or Moffit, 2003). The expectation of unambiguously positive effects on labour force participation is based on two conditions. First, the subsidies have to be targeted at individual income rather than household income, and second, the reform has to change the incentives to take up work for recipients of unemployment benefits or other social transfers. The subsidies under the German “Mini-Job” reform are indeed targeted at the individual level; however, the budget constraint for recipients of social transfers hardly changes due to strict withdrawal of earnings, as is shown in Steiner

¹For a detailed overview of recent European “Making Work Pay” policy reforms, see Orsini (2006).

²See Steiner and Wrohlich (2005) for a more detailed description of the reform. Note that the reform did not change employers’ SSC except for mini-jobs in private households. Hence, there is no reason for a marked increase in the overall demand for marginal employment.

and Wrohlich (2005). Additionally, it should be noted, that without a *minimum* hours restriction, the reform provides subsidies for all kinds of low-earnings jobs, whether low earnings result from low wages or short working hours. Hence, the reform cannot be directly compared to programmes such as the WFTC. The differences between the “Mini-Job” reform and the other mentioned reforms are even more pronounced when one looks at the incentives to take up a secondary job. After the reform, earnings from a secondary job—defined as a job which is held in addition to a primary job bound to social security contributions—are not only exempted from social security contributions but also from the income tax. Thus, incentives to take up such a job have markedly increased.

Already very shortly after its introduction, the “Mini-Job” reform was portrayed by different official sources as quite successful in generating new employment. The Federal Ministry of Health and Social Affairs stated in July 2003 that three months after the reform, 930,000 new jobs had been created.³ These numbers were corrected by the Federal Employment Agency in November 2003, who stated that one month after the reform, there was an increase in marginal employment of as high as 79,000 individuals and an increase of secondary jobs by 580,000. However, it is not clear whether this increase is *causally* related to the reform, since based on the theoretical reasoning above—if at all—only small effects can be expected. Hence, an ex-post evaluation is called for. This is an especially difficult task here, since from 2004 onwards various other legal changes have been introduced which might affect labour supply decisions of individuals simultaneously. Therefore, it should be obvious that a comparison of the mini-jobs realised in 2004 with pre-reform numbers will not reveal the true effect of the reform. Furthermore, in contrast to other evaluation studies of labour market policies, the distinction between control and treatment groups is not initially clear, since the reform is relevant for the whole population. A thorough evaluation has to take these points into consideration and should be based on a credible identification strategy. We will do so by using the exogenous variation in the interview date of the German Socio-Economic Panel (SOEP). The interviews are conducted between January and October in each year. Since the reform was introduced on April 1, 2003, we observe some people who are interviewed before the reform and others interviewed

³See press-release of the “Mini-Job-Zentrale” from July 18th, 2003: “930,000 neue Jobs durch geringfügig Beschäftigte”.

after the new legislation was implemented. This allows us to estimate the immediate short-run effect of the reform. To account for seasonal variation, we additionally use a difference-in-differences approach.

Our results show that, although there has been a rise in marginal employment after the introduction of the “Mini-Job” reform, this rise cannot be *causally* related to the reform. In the short run, we do not find a significant effect of the reform on marginal employment. However, we do find evidence for a positive effect on secondary job-holding for single men. We will explain our identification strategy in more detail in Section 2, where we will also describe the data used for the analysis. Section 3 contains the estimation results, before Section 4 concludes.

2 Evaluation Strategy and Data

Our empirical analysis is based on the German Socio-Economic Panel (SOEP), a sample gathering socio-demographic and financial information about 12,000 representative households each year. We will use the waves for the years 2002 and 2003. The individuals are interviewed in person from January until October each year.⁴ Our identification of the treatment effect of the reform will be based on this exogenous variation in the interview month.

2.1 Evaluation Design

As already mentioned we want to evaluate the effects of the reform on some outcome Y , for example, the probability of beginning a mini-job for certain groups of the population. In the usual microeconomic evaluation framework (the “potential outcome approach”, most commonly called the Roy (1951)-Rubin (1974) model), the treatment effect Δ is given by a comparison of the treatment outcome (Y^1) with a hypothetical situation where the same individual does not receive treatment (Y^0), i.e.: $\Delta = Y^1 - Y^0$. The fundamental evaluation problem arises because we can never observe both potential outcomes for the same individual at the same time. A simple comparison between outcomes of treated and untreated individuals is not possible if they are selective groups, that is when the condition $E(Y^0 | D = 1) = E(Y^0 | D = 0)$

⁴For a detailed description of the data, see Haiken De-New and Frick (2003).

does not hold, where D is a binary treatment indicator. Let us transfer this general framework to our evaluation question, before we present our identification strategy.

The “Mini-Job” reform was introduced on April 1, 2003, and applies to the whole population. Hence, we have no direct treatment group which has received the treatment and whose outcome we could compare with a control group who did not receive the treatment.⁵ The whole population before April 1, 2003, was not affected by the reform, while the whole population after April 1, 2003, was affected by it. It should also be noted that the whole population was (not) affected by the reform in 2004 (2002). Comparing the outcomes between these two years ($Y_{2004}^1 - Y_{2002}^0$) will not give us the actual treatment effect, since other regulations were also changed. Most significant of these were changes in the income tax as part of the German Tax Reform. From 2003 to 2004, the basic allowance was increased from 7,235 to 7,664 euro per year, the tax rate of the first tax bracket was reduced from 19.9 to 16.0 percent, and the top tax rate was reduced from 48.5 to 45.0 percent. Clearly, this reform also affected labour supply decisions of individuals with low earnings.⁶

However, the timing of the SOEP interviews gives us an opportunity to identify the true treatment effect. As mentioned above, the SOEP interviews are conducted between January and October of each year. We argue that the random variation of the interviews mimics a natural experiment, where we can compare the effects for the group of participants, i.e. the people who were interviewed when the reform was already implemented in t_{2003} , with the group of controls, i.e. the people who were interviewed before the reform was implemented in $t_{2003'}$ ⁷:

$$\Delta = Y_{2003}^1 - Y_{2003'}^0. \quad (1)$$

Most of the interviews are accomplished within the first quarter. In fact, by default households are contacted by the interviewers in the first quarter of each year. If this contact is not successful, whether because no one is at home or the household has moved to another address, households are contacted again in the second quarter of the

⁵Recent examples for evaluation studies with a ‘classical’ distinction between treatment and control groups can be found in Andren and Andren (2006), Fertig (2007) or Caliendo, Hujer, and Thomsen (2007).

⁶For a detailed description and an estimation of labour supply reactions to this tax reform see Haan and Steiner (2005). They show that total hours worked increase by about 1% due to this reform.

⁷The superscript ‘ ’ behind year information indicates the first quarter of the year, year information without superscript indicates quarters 2-4.

year and so on. Since on average most of the post-reform interviews are completed by May 2003⁸, it should also be clear that we are only able to estimate the immediate short-run effects of the reform.

A problem which might arise with this approach are potential differences in unobserved characteristics (UC) between individuals interviewed before April and those interviewed after April as well as seasonal employment effects (SEE). If employment in the mini-job sector varies heavily within a year or if the two groups differ in unobserved characteristics the above-mentioned approach becomes invalid since

$$Y_{2003}^1 - Y_{2003'}^0 = \Delta + SEE + UC. \quad (2)$$

To account for these potential sources of bias, we apply a control mechanism based on the difference-in-differences (DID) approach⁹, using the seasonal variation and unobserved differences in the year 2002 to account for the seasonal variation and unobserved differences in 2003. Clearly, this assumption is only valid if both patterns have not changed over the two years, such that $SEE_{2003} = SEE_{2002}$ and $UC_{2003} = UC_{2002}$.¹⁰ The treatment effect is then given by (see also Figure 1):

$$\Delta = (Y_{2003}^1 - Y_{2003'}^0) - (Y_{2002}^0 - Y_{2002'}^0). \quad (3)$$

Since we are using cross-sectional information from two waves of the SOEP, the populations in 2002 and 2003 as well as the populations in the first and subsequent quarters might not be the same. To account for variations in observable characteristics, we specify the outcome variable Y in a parametric way and estimate the effect on the whole sample with interaction effects.

The equation we will estimate can be specified as

$$y_i^* = \beta_1 * d2003_i + \beta_2 * after_i + \beta_3 * d2003 \times after_i + \gamma' X_i + \varepsilon_i, \quad (4)$$

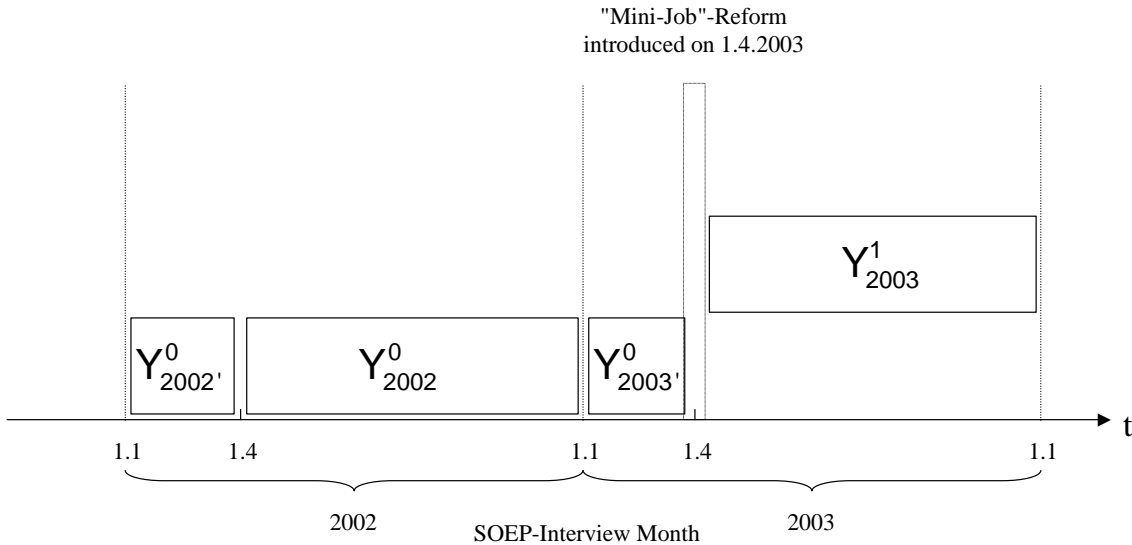
where y_i^* is a latent variable such as the propensity to be marginally employed or to hold a secondary job (the outcome variables will be specified in more detail in Section

⁸In 2003 80% of interviews were conducted in the first quarter, 8% in April, 5% in May, 4% in June and the rest (3%) between July and October.

⁹See, for example, Heckman, LaLonde, and Smith (1999) for an overview.

¹⁰Looking at the GDP growth between 2002 and 2003 shows that there has been only a small change of 0.8% between these two years. More importantly, the change between the first and second quarter in both years was about 2%.

Figure 1: Definition of the Subsamples According to the SOEP Interview Month



2.2), $d2003_i$ is a dummy variable indicating whether individual i is observed in 2003, $after$ is a dummy variable indicating whether individual i is observed after the first quarter of a year, and $d2003 \times after$ is an interaction term of these two variables. Vector X_i summarises control variables such as age, education, family status, number of children, health status, etc., and ε_i is an unobserved error term. The β s and the vector γ include the respective coefficients. We are particularly interested in β_3 , which yields the causal effect of the reform. Since we do not observe the latent variable y_i^* , but the binary outcome variables y_i , we will estimate equation (4) using a probit model. The marginal effect corresponding to the coefficient β_3 can thus be interpreted as change in the probability of the outcome variable (e.g. marginal employment) due to the reform.

It might be argued that this evaluation design leads to a downward bias of our results since the “Mini-Job” reform has been announced a few months before its introduction. Thus, individuals might have started to *search* for a job already before April 2003. We argue, though, that even if people started to search for a job before April 2003 there was no incentive to *start* the job before April 1st under the old legislation. Thus, when asked about their employment status, we do not expect an effect for those individuals interviewed before April 1st, 2003.

2.2 Outcome Variables, Subgroups and Some Descriptive Statistics

We are interested in two outcome variables, namely the probability of being in marginal employment (“geringfügige Beschäftigung”) and the probability of having a secondary job (“Nebenerwerbstätigkeit”), since the incentives to take up these two types of jobs have been changed by the “Mini-Job” reform. Examining the effects on secondary job holding seems to be especially relevant, since there is evidence that the strong increase in mini-jobs after the introduction of the reform was not caused by new jobs taken up by individuals who were previously not employed. Instead it is likely that a share of the new mini-jobs were jobs taken up by individuals who had already been working.¹¹ Thus, in addition to the analysis of marginal employment, we are particularly interested in the effect of the reform on secondary job holding. Furthermore, we also analyse the effect on the labour supply of students. This group was even more affected by the reform, because the SSC-exemption threshold is at the same time the exemption limit of earned income for recipients of student aid.¹²

In the SOEP data, there are several questions containing information about employment status, working hours, earnings, and job characteristics. We use the following definitions for the two outcome variables of interest. An individual is defined to be in marginal employment if

- the answer to employment status is “marginally employed”, or
- the answer to employment status is “part-time employed” and gross monthly earnings are reported to be less than 400 euro, or
- the answer to job characteristics is “this job is a 325 euro/400 euro Job”, or
- the answer to employment status is “not working” and the individual reports having a secondary job with gross monthly earnings less than 400 euro.

Note that we use the post-reform threshold of 400 euro for all individuals (interviewed before and after the reform), since we are not interested in redefinitions of already existing jobs.

¹¹For details see Bundesagentur für Arbeit (2003).

¹²Note that jobs with a monthly income between 400 and 800 Euro might become more attractive under the reform as well. However, in this paper we only look at “marginal employment”, which refers to jobs with an income up to 400 Euro per month.

Relating to the second outcome variable, we define an individual as holding a secondary job if

- the answer to employment status is “full-time employed” or “part-time employed” and the individual reports gross monthly earnings from regular or irregular secondary jobs less than 400 euro.

Table 1 shows the total number of observations in the four subsamples, interviewed before and after April 1st in 2002 and 2003, respectively. To analyse the changes with respect to marginal employment we look at the whole population, whereas we focus on individuals who are full- or part-time employed to analyse secondary job-holding. For both analyses we focus on individuals aged between 16 and 70 years.

Results of several ex-ante evaluation studies (see Steiner and Wrohlich, 2005, or Bargain, Caliendo, Haan, and, Orsini, 2006) have shown that the reaction to the reform differs between population subgroups. For example, women in couple households have been shown to adjust their labour supply to a greater extent than men. Therefore, we differentiate between several subgroups in our analysis. In particular, we run separate estimations for men, women and students and include interaction terms for individuals living in single and couple households.

Table 1: Number of Observations, Marginal and Secondary Employment in the Subsamples

Group	Subsample (Interviewed in...)			Marginal Employment			Secondary Employment		
				Obs.	Abs.	Share	Obs.	abs.	Share
Men	2002	before	April 1st	7007	267	0.0381	4372	160	0.0366
		after		2240	91	0.0406	1530	59	0.0386
	2003	before	April 1st	7102	313	0.0441	4367	168	0.0385
		after		1805	99	0.0548	1199	53	0.0442
Women	2002	before	April 1st	7366	872	0.1184	3422	119	0.0348
		after		2405	352	0.1464	1178	43	0.0365
	2003	before	April 1st	7527	1020	0.1355	3494	118	0.0338
		after		1917	307	0.1601	956	35	0.0366

Note: The high income sample of the SOEP is not included, since this entire group was interviewed after April in the 2003 wave. Numbers for marginal employment refer to the whole population between 16 and 70 years old. Numbers for secondary employment refer to the whole population holding a full- or part-time job (between 16 and 70 years old).

Source: SOEP, waves 2002 and 2003.

Table 1 also shows the total number of observations marginally employed or holding a secondary job. We see that marginal employment is more prevalent among

women than men, whereas the opposite is true for secondary job holding. Additionally, marginal employment is higher in the second and third quarter of both years when compared to the first quarter. Furthermore, it is interesting to note that we observe an increase in marginal employment from the first quarter of 2002 to the first quarter of 2003.

Before we turn to the estimation results, we look at some descriptives of the covariates used in the estimations. Thereby we differentiate the four subsamples under consideration. As can be seen from Table 1, there are far fewer observations in the two subsamples “interviewed after April 1st”, which is due to the interview routine of the SOEP already described. Therefore, the group of individuals interviewed after April 1st might be systematically different from those interviewed in the first three months.

Table 2: Some Descriptive Statistics - Differentiated by Interview Date

	2002		2003		Test of mean equality ¹ (p-value)
	Before April 1st	After April 1st	Before April 1st	After April 1st	
Age	43.62	41.22	43.64	41.61	0.088
Female	0.512	0.518	0.515	0.515	0.323
No Educational Degree	0.017	0.026	0.018	0.024	0.205
High School Degree	0.201	0.240	0.207	0.243	0.327
Vocational Training	0.658	0.618	0.657	0.615	0.389
Academic	0.170	0.191	0.172	0.186	0.197
Disabled	0.096	0.074	0.095	0.078	0.204
Married	0.640	0.613	0.623	0.625	0.000
Single	0.170	0.183	0.175	0.178	0.087
Cohabiting Couple	0.203	0.223	0.214	0.219	?!?
German	0.919	0.864	0.923	0.862	0.176
Living in East Germany	0.285	0.139	0.290	0.105	0.000
Children under 15	0.329	0.357	0.318	0.361	0.046

Source: SOEP, waves 2002 and 2003.

¹ *p*-values refer to a test of the following hypothesis: $H_0 : (X_{2003} - X_{2003'}) - (X_{2002} - X_{2002'}) = 0$.

Note that—as already discussed—even if the “after” groups differ systematically from the “before” groups with respect to unobservable characteristics, this does not flaw our results as long as we assume that the differences are the same in 2002 and 2003. The only assumption that needs to be valid for our study is that the interview month of the 2003 wave is independent of the introduction of the “Mini-Job” reform on April 1, 2003. We tested the significance of the differences across the four subsamples using a t-test of mean equality. Thereby we tested the hypothesis that the difference-in-differences of the covariates equals zero, i.e., $H_0 : (X_{2003} - X_{2003'}) - (X_{2002} - X_{2002'}) = 0$.

Looking at the p -values in column 5 of Table 2 shows that the differences are in fact not different from zero for most of the variables. Exceptions (at the 5%-significance level) are the marital status, living in East Germany¹³ and children under 15. To take care of these small remaining differences, we control for the covariates in the parametric estimation of the treatment effect in the next section.

3 Results

Since we expect different effects for men and women as well as for students, we perform separate analyses for these groups. For men and women, we run estimations for two outcome variables, namely marginal employment (Section 3.1) and secondary job holding (Section 3.2). For students we combine these two outcome variables into one due to the limited number of observations (Section 3.3). For all subgroups and outcome variables, we run three probit estimations, respectively. The first estimation is only for the year 2003, and includes a dummy indicating “interviewed after April 1st” as a single explanatory variable. This corresponds to the “raw” effect of the reform, without controlling for potential seasonal effects or possible differences in observable or unobservable characteristics between individuals interviewed in and after the first quarter of 2003. In the second estimation, we control for differences in observable characteristics by including a set of control variables such as age, educational variables, regional variables, marital status, and number of children. Finally, in the third estimation we pool data from 2002 and 2003, and include two more variables, a dummy indicating the year 2003 and an interaction term between this year dummy and the “interviewed after April 1st” dummy (see equation 4). Note that this variable measures the effect that the “Mini-Job” reform had on the outcome variable, controlling for seasonal effects, observable and time-invariant unobservable characteristics that differ between the groups interviewed in and after the first quarter of each year.

¹³Given the differences in this variable, we have also performed the analysis separately for East and West Germany, which did not yield different results from the ones we will present in the next section.

3.1 Effects on Marginal Employment

Table 3 shows a short summary of the estimation results for marginal employment, where we have displayed marginal effects. Full estimation results, including the coefficients and standard errors of the control variables, can be found in Table A.1 in the Appendix. For men, the first model indicates that the “raw” effect of the reform is positive, i.e. in the second and third quarter of 2003 we observe more men in marginal employment than in the first quarter of 2003. This is still true if we control for differences in observable characteristics, as can be seen from column 2. This is in line with the descriptive statistics showing a post-reform increase in marginal employment. However, to answer the question whether this increase is *causally* related to the reform we have to look at the third column. The third column shows the estimation of the pooled sample of 2002 and 2003 including a dummy variable indicating the year 2003, and an interaction term of this dummy with the dummy indicating “interviewed after the reform”. We further interacted this variable with the “single” dummy, because ex-ante studies have shown different reactions to the reform by singles and individuals living with a partner. Doing so allows us to calculate marginal effects for singles and couples separately.¹⁴ Our results show, that as far as the probability of being marginally employed is concerned, neither single men nor men living in couples react to the reform.

Similar to what we observe for men, we find a positive and significant “raw” effect of the reform for women (see column 4). This effect disappears, however, once we control for socio-demographics (column 5) and for differences in seasonal employment effects and unobservable characteristics between the “before” and “after” samples (column 6). Note that in the full model presented in column 6, the coefficients of the variables *after* and *d2003* are positive and significant, indicating that for women the probability of being marginally employed is higher in the second and third quarter of each year, and that this probability is also higher in 2003. However, there is no causal effect of the reform, which would be caught by the effect of the variables *after* \times 2003 and

¹⁴Note that the marginal effect of the interaction term is not equal to the magnitude of the interaction effect in non-linear models (see Ai and Norton, 2003). In order to calculate the absolute treatment effect for couples and singles, we use the following formula: $\Phi(\hat{\beta}_{after} + \hat{\beta}_{2003} + \hat{\beta}_{after2003} + \hat{\gamma}'\bar{X}) - \Phi(\hat{\beta}_{after} + \hat{\gamma}'\bar{X}) - \Phi(\hat{\beta}_{2003} + \hat{\gamma}'\bar{X}) + \Phi(\hat{\gamma}'\bar{X})$ where Φ is the cdf of the normal distribution. For singles, the marginal effect corresponds to $\Phi(\hat{\beta}_{after} + \hat{\beta}_{2003} + \hat{\beta}_{single} + \hat{\beta}_{after2003} + \hat{\beta}_{after2003single} + \hat{\gamma}'\bar{X}) - \Phi(\hat{\beta}_{after} + \hat{\beta}_{single} + \hat{\gamma}'\bar{X}) - \Phi(\hat{\beta}_{2003} + \hat{\beta}_{single} + \hat{\gamma}'\bar{X}) + \Phi(\hat{\beta}_{single} + \hat{\gamma}'\bar{X})$. The corresponding standard errors are calculated using the Delta method.

Table 3: Estimation Results (Marginal Effects) - Marginal Employment

Variable	Men			Women		
	Model 1	Model 2	Model 3 ^(a)	Model 1	Model 2	Model 3 ^(a)
after	0.0108*	0.0132**	0.0036	0.0246***	0.0062	0.0171**
	(0.0059)	(0.0055)	(0.0045)	(0.0093)	(0.0087)	(0.0080)
d2003			0.0045*			0.0195***
			(0.0024)			(0.0041)
after×2003 (Couples)			0.0090			-0.0092
			(0.0067)			(0.0108)
after×2003 (Singles)			0.0145			-0.0158
			(0.0135)			(0.0247)
Controlled for Covariates	no	yes	yes	no	yes	yes
Log-Likelihood	-1666.813	-1529.5	-2919.3	-3829.5	-3664.7	-7183.4
Observations	8,907	8,907	18,154	9,444	9,444	19,215

Note: ***/**/* indicates significance at the 1%/5%/10% level. Standard errors (in parentheses) correct for correlation across repeated observations of individuals.

^(a) Model 3 has also been estimated without the interaction term ‘after×2003 (Singles)’. The marginal effect for men is 0.0084 (s.e. 0.0065), for women -0.0105 (s.e. 0.0094). Covariates include: age, age², no education, high school degree, vocational training, academic, disabled, married, single, german, number of children in different age classes, and a dummy for living in East Germany. See also Table A.1.

Source: Estimations based on SOEP, waves 2002 and 2003.

after × 2003 × *single* (see Table A.1 in the Appendix). To check whether certain subgroups of the population were particularly affected by the reform, we have, for example, also included interaction terms of *after* × 2003 and *low – skilled* in separate estimations. However, we did not find any significant effects.¹⁵ Moreover, we evaluated the marginal effects not only at the sample mean, but also for several subgroups (e.g. with respect to education, marital status and number of children) which did not change our results.

Thus, our first conclusion is that in the short-run (defined as about two months after the reform), there has not been a significant change in marginal employment that could be causally related to the legislation introduced on April 1st, 2003. However, at least for women, marginal employment seems to be higher in the summer months than in winter and higher in 2003 than in 2002.

3.2 Effects on Secondary Job Holding

Let us now turn to the analysis of the probability of holding a secondary job. As already explained above, for these estimations we focus on the sample of full-time or

¹⁵These estimation results are available on request from the authors.

part-time employed individuals only. As Table 4 (column 1) shows, there seems to be no significant “raw” effect of the reform for men, as the share of men holding a secondary job does not differ between the first and the subsequent quarters of 2003.¹⁶ This is also true if we control for differences in observed characteristics (column 2) and for differences in seasonal employment effects and time-invariant unobserved characteristics (column 3).

Table 4: Estimation Results (Marginal Effects) - Secondary Employment

Variable	Men			Women		
	Model 1	Model 2	Model 3 ^(a)	Model 1	Model 2	Model 3 ^(a)
after	0.0058 (0.0066)	0.0051 (0.0062)	0.0012 (0.0054)	0.0030 (0.0068)	0.0004 (0.0062)	0.0001 (0.0057)
d2003			0.0017 (0.0030)			-0.0009 (0.0033)
after×2003 (Couples)			-0.0015 (0.0072)			0.0017 (0.0087)
after×2003 (Singles)			0.0311 (0.0198)			-0.0023 (0.0207)
Controlled for Covariates	no	yes	yes	no	yes	yes
Log-Likelihood	-929.1	-891.6	-1794.2	-665.8	-644.4	-1324.8
Observations	5,564	5,564	11,466	4,447	4,447	9,047

Note: ***/**/* indicates significance at the 1%/5%/10% level. Standard errors (in parentheses) correct for correlation across repeated observations of individuals.

^(a) Model 3 has also been estimated without the interaction term ‘after×2003 (Singles)’. The marginal effect for men is 0.0031 (s.e. 0.0072), for women 0.0009 (s.e. 0.0078).

Covariates include: age, age², no education, high school degree, vocational training, academic, disabled, married, single, german, number of children in different age classes, a dummy for living in East Germany, industry class, full-time employment dummy, and overtime. See also Table A.2.

Source: Estimations based on SOEP, waves 2002 and 2003.

However, as the results of this estimation show (see Table A.2 in the Appendix), we do find a positive effect for single men that is significant at the 10 percent level. The marginal effect corresponding to this coefficient amounts to 0.031. This implies that for single men, the probability of having a secondary job increases by 3.1 percentage points. Since the probability of holding a secondary job before the reform for single men is 3.7 percent, this effect almost implies a doubling of secondary employment in this group. However, the standard error of the marginal effect amounts to 0.0198.¹⁷ The marginal effect is thus not significant at the 10 percent level, the empirical significance level amounting to 11.5%. Given the economic significance of the effect and the

¹⁶Full estimation results can be found in Table A.2 in the Appendix.

¹⁷Statistical (non)significance of the estimated coefficient of the interaction term does not necessarily imply (non)significance of the marginal effect of this variable in non-linear models (see Ai and Norton, 2003).

relatively limited number of observations, we would not conclude from the standard error that the reform did not affect this group, but rather that there is evidence for a positive effect on secondary employment of single men. As columns 4 to 6 of Table 4 show, we do not find a corresponding effect for women.¹⁸

3.3 Effects for Students

The estimation results for students can be found in Table 5.¹⁹ Similar to what we found for women with respect to marginal employment, for students there is a positive and significant “raw” effect of the reform. Students in the second and third quarter of 2003 are more likely to be observed in marginal employment or holding a secondary job than in the first quarter of 2003.

Table 5: Estimation Results (Marginal Effects) - Marginal and/or Secondary Employment for Students

Variable	Students		
	Model 1	Model 2	Model 3
after	0.0639*** (0.0220)	0.0500** (0.0220)	0.0465** (0.0193)
d2003			0.0379*** (0.0112)
after×2003			0.0042 (0.0267)
Controlled for Covariates	no	yes	yes
Log-Likelihood	-1161.7	-1134.1	-2197.5
Observations	2,295	2,295	4,703

Note: ***/**/* indicates significance at the 1%/5%/10% level. Standard errors (in parentheses) correct for correlation across repeated observations of individuals. Covariates include: age, age², no education, high school degree, vocational training, academic, disabled, married, single, german, number of children in different age classes, and a dummy for living in East Germany. See also Table A.4. *Source:* Estimations based on SOEP, waves 2002 and 2003.

This is still true once we control for socio-demographic characteristics. The difference-in-differences model, however, shows that there is no causal effect of the reform, even

¹⁸Once again we evaluated the marginal effects also for several subgroups. In none of these cases we could find a significant effect.
¹⁹Table A.3 in the Appendix contains the total number of observations for this group as well as the numbers on being marginally employed and/or holding a secondary job. Due to the limited number of observations we pooled male and female observations and included a control variable for gender. Full estimation results can be found in Table A.4 in the Appendix.

though the probability of being marginally employed or holding a secondary job is higher in 2003 and in the second and third quarter of each year.²⁰

To sum up, we find that in the short run, there is evidence that the reform had a causal effect for single men, whose probability of having a secondary job increases by about three percent. According to our estimation results, the reform had no causal effect on marginal employment in any of the subgroups, although we do find a general rise in marginal employment in the second quarter of 2003 as compared to previous periods.

4 Conclusions

The aim of this paper was to evaluate the causal effect of the German “Mini-Job” reform from 2003 on the probabilities of being in marginal employment or of having a secondary job. Based on our identification strategy, we were able to identify the short-run effects of the reform. Disentangling seasonal or cyclical trends from causal effects is a major requirement in order to draw policy-relevant conclusions. For example, we find that for women and students marginal employment is higher in the summer months (compared to the first quarter) and in the year 2003 (compared to the year 2002). This increase might have been reflected in the numbers—a short-run increase of 79,000 jobs—reported by the Federal Employment Agency. However, we show that this increase can not be *causally* related to the reform. As far as secondary jobs are concerned, our results differ to a much larger extent from the numbers published by the Federal Employment Agency. We only find evidence for a positive reaction to the reform among single men, whose probability of having a secondary job increases. However, this can not explain the total increase of 580,000 secondary jobs stated above. We believe that a large fraction of these “new” jobs are actually redefinitions of previously fake/false self-employment, i.e., a situation where a self-employed individual is actually dependent on one company for (most of) her income. This effect cannot be identified with the SOEP data. The same is true for turning illegal jobs into legal employment (see also Schupp and Birkner, 2004).

All ex-ante evaluation studies using behavioural microsimulation models predict

²⁰We also ran the same model including the variable $after2003 \times single$, which did not change the results.

similar effects from the “Mini-Job” reform suggesting only very moderate participation effects and even negative effects on working hours.²¹ They do find a small yet significant effect on the labour force participation of women living in couple households. As described above, we do not find a significant effect on the participation in marginal employment in the short-run. However, since the effects that are calculated with ex-ante microsimulation techniques correspond to long-term effects, our results need not necessarily be a contradiction to this literature.

Even though our results only reflect the short-run effects of the “Mini-Job” reform, they are in line with what could be expected according to its set-up. As we have pointed out, the reform was not explicitly targeted to subsidize employment in the low-wage sector since there is no minimum hours restriction. Moreover, working incentives for individuals receiving unemployment benefits or social assistance have hardly been changed due to the strict withdrawal rate of earnings. The largest change was actually the exemption from income taxation for earnings up to 400 Euro from a secondary job. Hence, it is also questionable whether the reform will achieve its objective—to provide positive work incentives for people with low earnings potential—in the long-run.

²¹See e.g. Steiner and Wrohlich (2005), Arntz, Feil, and Sperrmann (2003) or Bargain, Caliendo, Haan, and Orsini (2006).

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Appendix - Tables

Table A.1: Estimation Results (Coefficients) for Men and Women - Marginal Employment - Full Model

Variable	Men			Women		
	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3
after	0.106*	0.148**	0.05	0.107***	0.029	0.083**
	(0.055)	(0.059)	(0.058)	(0.039)	(0.04)	(0.038)
d2003			0.062*			0.097***
			(0.032)			(0.020)
after × 2003			0.095			-0.05
			(0.081)			(0.051)
after × 2003 × single			0.029			-0.021
			(0.132)			(0.099)
age		-0.121***	-0.119***		0.025***	0.018**
		(0.011)	(0.009)		(0.009)	(0.007)
age squared		0.001***	0.001***		-0.000***	-0.000***
		(0.000)	(0.000)		(0.000)	(0.000)
no education		-0.293	-0.225		-0.202	-0.215*
		(0.228)	(0.163)		(0.131)	(0.120)
high-school degree		0.392***	0.349***		0.067	0.021
		(0.065)	(0.057)		(0.048)	(0.043)
vocational training		-0.026	-0.057		-0.156***	-0.163***
		(0.062)	(0.049)		(0.039)	(0.034)
academic		-0.221***	-0.258***		-0.358***	-0.364***
		(0.080)	(0.075)		(0.058)	(0.054)
disabled		-0.029	0.041		-0.307***	-0.234***
		(0.084)	(0.073)		(0.075)	(0.06)
single		-0.006	-0.022		-0.013	0.01
		(0.073)	(0.062)		(0.055)	(0.047)
married		-0.222***	-0.206***		0.195***	0.220***
		(0.079)	(0.063)		(0.053)	(0.043)
german		-0.033	0.014		0.184***	0.219***
		(0.093)	(0.077)		(0.062)	(0.055)
children under 15		-0.033	-0.056			
		(0.065)	(0.052)			
children under 1					-0.234**	-0.199***
					(0.099)	(0.074)
children under 7					0.069	0.058
					(0.048)	(0.039)
children between 8-15					0.185***	0.202***
					(0.040)	(0.033)
east german		0.039	0.032		-0.309***	-0.344***
		(0.057)	(0.049)		(0.043)	(0.037)
constant	-1.705***	0.598***	0.486***	-1.101***	-1.493***	-1.495***
	(0.026)	(0.218)	(0.182)	(0.018)	(0.168)	(0.139)
Log-Likelihood	-1666.8	-1529.5	-2919.3	-3829.5	-3664.7	-7183.4
Observations	8,907	8,907	18,154	9,444	9,444	19,215

Note: ***/**/* indicates significance at the 1%/5%/10% level. Standard errors (in parentheses) are corrected for correlation across repeated observations of individuals.

All variables except age and age squared are dummy variables, taking the value 1 if the condition is fulfilled.

Source: Estimations based on SOEP, waves 2002 and 2003.

Table A.2: Estimation Results (Coefficients) for Men and Women - Secondary Employment - Full Model

Variable	Men			Women		
	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3
after	0.066 (0.072)	0.065 (0.076)	0.015 (0.071)	0.038 (0.086)	0.006 (0.090)	0.001 (0.081)
d2003			0.023 (0.039)			-0.012 (0.047)
after×2003			-0.02 (0.096)			0.024 (0.122)
after×2003×single			0.320* (0.172)			-0.041 (0.179)
age		0.02 (0.025)	0.043** (0.021)		0.001 (0.027)	0.003 (0.020)
age squared		-0.000 (0.000)	-0.001** (0.000)		-0.000 (0.000)	-0.000 (0.000)
no education		-0.058 (0.415)	-0.404 (0.387)		0.506* (0.294)	0.38 (0.251)
vocational training		0.234*** (0.087)	0.113 (0.07)		-0.013 (0.092)	-0.059 (0.074)
academic		0.108 (0.084)	0.091 (0.07)		0.185* (0.099)	0.195** (0.081)
disabled		0.067 (0.144)	0.072 (0.116)		-0.154 (0.203)	0.046 (0.140)
married		0.131 (0.098)	0.077 (0.082)		-0.124 (0.107)	-0.038 (0.085)
single		0.117 (0.103)	-0.028 (0.096)		0.163 (0.107)	0.242*** (0.083)
german		0.164 (0.133)	0.166 (0.112)		-0.096 (0.143)	0.027 (0.120)
children under 15		-0.08 (0.079)	-0.059 (0.065)			
children under 1					-0.072 (0.123)	-0.100 (0.097)
children between 8-15					-0.137 (0.098)	-0.108 (0.075)
east german		-0.121 (0.082)	-0.142** (0.070)		-0.156 (0.097)	-0.122 (0.076)
civil servant		0.119 (0.116)	0.084 (0.099)		-0.320* (0.180)	-0.320** (0.134)
self-employed		-0.377*** (0.138)	-0.293*** (0.110)		-0.18 (0.184)	-0.255 (0.163)
industry class. 2		-0.107 (0.108)	-0.057 (0.093)		0.046 (0.176)	-0.066 (0.166)
industry class. 3		0.031 (0.122)	0.124 (0.099)		-0.101 (0.142)	-0.148 (0.112)
industry class. 4		-0.181 (0.160)	-0.165 (0.126)		-0.326 (0.296)	-0.264 (0.248)
industry class. 5		0.184** (0.093)	0.231*** (0.079)		0.169 (0.107)	0.134 (0.090)
industry class. 6		0.109 (0.119)	0.196** (0.099)		-0.112 (0.155)	-0.020 (0.116)
industry class. 7		0.16 (0.139)	0.139 (0.115)		0.275* (0.151)	0.257** (0.124)

Continued on next page.

Table A.2 continued.

Variable	Men			Women		
	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3
overtime ($< 3h$)		-0.056 (0.087)	0.027 (0.067)		0.105 (0.087)	0.123* (0.064)
overtime ($\geq 3h$)		0.147** (0.075)	0.163*** (0.059)		-0.024 (0.109)	0.007 (0.077)
full-time employed		-0.507*** (0.140)	-0.560*** (0.109)		-0.069 (0.085)	-0.148** (0.068)
constant	-1.769*** (0.035)	-1.930*** (0.502)	-2.348*** (0.402)	-1.828*** (0.041)	-1.693*** (0.532)	-1.797*** (0.397)
Log-Likelihood	-929.1	-891.6	-1794.2	-665.8	-644.4	-1324.8
Observations	5564	5564	11466	4447	4447	9047

Note: ***/**/* indicates significance at the 1%/5%/10% level. Standard errors (in parentheses) are corrected for correlation across repeated observations of individuals.

All variables except age and age squared are dummy variables, taking the value 1 if the condition is fulfilled.

Source: Estimations based on SOEP, waves 2002 and 2003.

Table A.3: Number of Observations, Marginal and/or Secondary Employment for Students in the Subsamples

Subsample			Obs.	Marg. or Secon. Employment	
				abs.	in %
2002	before	April 1st	1778	274	0.0574
	after		630	132	0.0873
2003	before	April 1st	1819	350	0.0808
	after		476	122	0.1324

Note: High income sample of the SOEP is not included, since this entire group was interviewed after April in the 2003 wave. Numbers refer to the population in "Ausbildung".

Source: SOEP, waves 2002 and 2003.

Table A.4: Estimation Results (Coefficients) for Students - Marginal and/or Secondary Employment - Full Model

Variable	Students		
	Model 1	Model 2	Model 3
after	0.214*** (0.071)	0.160** (0.073)	0.159** (0.068)
d2003			0.145*** (0.043)
after×2003			-0.005 (0.093)
age		0.060*** (0.021)	0.048*** (0.018)
female		0.204*** (0.061)	0.229*** (0.049)
age squared		-0.001** (0.000)	-0.001** (0.000)
no education			
high-school degree			
vocational training		-0.239** (0.096)	-0.247*** (0.077)
academic			
disabled		-0.514 (0.314)	-0.331 (0.207)
single		0.033 (0.075)	0.083 (0.060)
married		-0.388*** (0.136)	-0.351*** (0.105)
german		0.283** (0.126)	0.178* (0.102)
children under 15		-0.134* (0.071)	-0.06 (0.055)
east german		-0.224*** (0.071)	-0.246*** (0.058)
constant	-0.869*** (0.034)	-2.025*** (0.342)	-1.889*** (0.285)
Log-Likelihood	-1161.7	-1134.1	-2197.5
Observations	2,295	2,295	4,703

Note: ***/**/* indicates significance at the 1%/5%/10% level. Standard errors (in parentheses) are corrected for correlation across repeated observations of individuals.

All variables except age and age squared are dummy variables, taking the value 1 if the condition is fulfilled.

Source: Estimations based on SOEP, waves 2002 and 2003.

Reply to the comments on „Evaluating the German Mini-Job Reform Using a Natural Experiment“

First of all, we would like to thank the referee for his/her valuable comments which helped to improve the paper. We will explain below in details how we incorporated the suggestions or indicate why we did not do so.

Major Comments

(1) Given the setup of the reform, one of its main effects should be that all existing employment contracts are changed and split in two parts, with a small part of 400 Euros being freed of income and social security taxes. This is not discussed (though briefly mentioned on the bottom of p.8) nor analyzed in the paper. Why not?

→ We discuss that now in more detail on page 8f. In fact, our analysis of secondary job holding is especially designed to detect such effects.

(2) The authors' conclusions are predominantly based on observations in month 1 and 2 after the reform. It seems questionable that the effects of the reform are realized this fast given search, information, and administrative requirements connected to hiring. Are there any institutional reasons for not looking at the evidence of 2004? this might be a valuable addition.

→ Unfortunately, we cannot use information from the year 2004 for our analysis, since from 2004 onwards various other legal changes have been introduced which might affect labour supply decisions of individuals simultaneously. Most significant of these were changes in the income tax as part of the German Tax Reform, increasing the basic allowance and decreasing the initial tax rate (see page 5 for more details).

(3) If individuals who cannot be interviewed in the first quarter of the year systematically and in unobservable ways differ from others and if these differences are correlated to their response to the reform, the estimates are biased. The authors should explicitly state their assumption that the reform effect is identical for all, once the quarter of observation is conditioned out. Particularly in this application this assumption might matter, as those working on second jobs should be less likely to be met at home by interviewers. It is therefore not certain that the variation of interview months is random, as stated on p.5.

→ We discuss that in detail on page 6f. “A problem which might arise with this approach are potential differences in unobserved characteristics (UC) between individuals interviewed before April and those interviewed after April [...]”. To account for these differences we apply a control mechanism based on the difference-in-differences (DID) approach. „Clearly, this assumption is only valid if both patterns have not changed over the two years, such that $SEE_{2003} = SEE_{2002}$ and $UC_{2003} = UC_{2002}$ “.

(4) The treatment of single vs. married vs. couple status of individuals is confusing. (a) Table 2 might present the test of mean equality also for the omitted category.

→ We present that now, see page 10.

(b) The estimations use one omitted category for the level effects and a different one for the treatment effects. The discussion covers absolute effects without referring to reference groups. It should be clarified whether the estimates show the difference between singles and non-married couples or whether they discuss the absolute level of effects for singles.

The marginal effects presented in Table 3 refer to absolute effects for singles and couples, as we explain in footnote 14.

(c) It might be of interest to - at least in a footnote - present the joint treatment effect (for singles and couples) and its significance instead of only presenting model 3.

→ We present the joint treatment effect now in the tablenotes to Tables 3 and 4. It is insignificant in both cases.

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(5) The authors should explain their computation of the marginal effect as presented in footnote 13 (e.g. by referring to equation 3).
Ai and Norton (2003) show, that the magnitude of the interaction effect in non-linear models does not equal the marginal effect of the interaction term. Thus, we calculate the interaction effect using the formula stated in footnote 14 (formerly 13). We have clarified that in this footnote, see page 12.

Minor Comments

- (1) Some references to the German institutional background are difficult to understand:
explain "fake self-employment" (p.16).
→ We have rephrased that.
- (2) Is the Orsini 2006 reference cited in the paper?
→ Yes, in footnote 1 on page 1.
- (3) Some typos
- middle of p2 "maximum hours restriction"
- top of p5 "groups which"
- top of p8 "the strong increase ... was"
→ We have corrected all typos.